Discussion: Session 4A—The Changing Distribution of Deaths

Jean-Marie Robine

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Discussant Comments: Jean-Marie Robine

JEAN-MARIE ROBINE: Good afternoon. I'm really happy to be here with you and to be the discussant of this session. I enjoyed reading all three papers. I think all of them, or at least two of them, are talking about the compression of mortality and the compression of morbidity, and maybe because I read first the paper of Eric Stallard, I decided to go that way and to not really comment on the papers one by one, but discuss this topic, compression of mortality versus compression of morbidity. In fact, I think I agree with the three papers except on one point. It's relative to the paper of Jack Yue, and I will say a few words on that later. I like Eric Stallard's comment on the relationships between the compression of mortality and the compression of morbidity or disability. I think it's an important issue. There is a lot of confusion about that. So I think it's really important to come back to the concepts and to the definitions. I think several of you know that I love to do that. I love to go back each time to the original papers and to read them again and again, and if you read the paper of James Fries, released in 1980, clearly he's never talking about compression of mortality. This is not part of the universe of James Fries, and this is part of the problem, because he is effectively talking about the

compression of morbidity. As Eric Stallard said, the definition of compression of mortality and the definition of compression of morbidity are two different definitions. That's why it is complicated. In fact, when you read the paper of James Fries, he's talking about the *rectangularization* of the survival curve. He's talking about normal age at death and he is referring to a normal distribution of the ages at death. He's using a very specific language, and so we have to find where this idea of compression of mortality comes from. In fact, this comes from the paper by George Myers and Ken Manton from 1984. Essentially, four years after 1980, two papers have been published in *Gerontology*, one by James Fries defending his comprehension of morbidity, and one by Myers and Manton where they are challenging it and they are introducing this idea of compression of mortality.

We got then, as you said, these two definitions, which are really different. The compression of morbidity is a decrease in the number of years lived with morbidity at the end of life. In fact, and this is very interesting in the paper of James Fries, it is the area between the survival curve and the morbidity curve. The introduction of the morbidity curve is a genuine idea of the James Fries paper. For Myers and Manton, the compression of mortality is a decrease in the dispersion of the individual life spans

and, therefore, is a decrease in the mean or the standard deviation from the mean life span.

Thus we have these two definitions that are totally different in two different universes, and on the top of that, effectively, we are interested to see what relations exist between both. Of course, it's also interesting to find where these specific words—"rectangularization of the survival curve," "natural mortality curve" or "normal distribution"—came from in the paper of James Fries, and when you read his paper, it's not clear. I read all papers quoted by Fries. They are two very interesting references there. The first one is a reference to the book of Alex Comfort, T*he Biology of Senescence,* and obviously the idea of rectangularization of the survival curve is coming from the work of Alex Comfort. The second one is a paper of [Major] Greenwood and [J. O.] Irwin published in 1939. I read this paper thanks to Leonid Gavrilov, who gave me a copy of this paper maybe 10 years ago now. Obviously Fries got his idea of normal distribution and normal aging mortality in this paper.

The paper of Greenwood and Irvin is important because it is the only link known with the work of Wilhelm Lexis (1878), proposing three types of mortality: (i) infant mortality, following an inverse J-curve on the right, (ii) the normal mortality, and you can see it's really normally

distributed, corresponding to aging-related mortality on the left, and in between (iii) the premature (or accidental) mortality. Because these three types of mortality tend to overlap, the distribution of the individual life spans is not symmetrical. Only above the modal age at death, individual life spans are, or may be, normally distributed. Below the mode, normal (i.e., aging related) and premature mortality overlap. This is why Väinö Kannisto (2001) proposed to measure the standard deviation only above the mode. The purpose was to have a direct measure of the dispersion of the aging-related mortality, pushing aside premature mortality.

This last remark is immediately leading us to what is the best way to estimate the location of the mode, because as you know there is a lot of noise around the modal age at death. But it is no longer a big issue since Nadine Ouellette and Robert Bourbeau (2011) proposed a very nice way to resume the distribution of the individual life durations with a non-parametric approach. This is extremely important because we don’t need to know whether the distribution of the individual life durations is normally distributed or not, logistically distributed or not. Indeed, we know they are not. If we use a parametric approach, using for instance a normal or a logistic model, part of the variability will be driven by the model. We

have a nice demonstration of the interest of using this non-parametric approach with [Emily] Clay's work in the U.K. It is quite easy today to resume the distribution of the individual life durations and to locate the modal age at death because Ouelette and Bourbeau provide a software to do that.

Therefore, I don't think it's a big deal today to determine what is the modal age at death, but I agree with Jack, we still need to find the best way to measure the deviation from the mode, and I'm not sure the standard deviation above the mode proposed by Kannisto is the best way. We also need, and you were talking about that, to have a good indication about the maximum life span, a good estimation of the maximum life span, practical or theoretical.

This is the only point on which I'm disagreeing with one of the three authors. Jack, I disagree with what you said about the standard deviation above the mode, because what you said is not what Kannisto proposed. Always going back to the original paper, Kannisto proposed when computing the standard deviation above the mode to use all ages of death above the mode. But you are proposing to do that using an age range, which is something like the modal age at death, plus or minus K varying from 5 to 15. That means, it's clear on the table you presented at the

beginning, when you fix K to 5, you are only looking at the ages at death five years below and five years above the modal age at death. By construction, the deviation is extremely small and limited to 5, and this has nothing to do with the actual standard deviation. Therefore, I don't think you can use this measure and say it is a measure proposed by Kannisto. You cannot conclude from this that the proposal of Kannisto is bad or not as good as the other measures you are proposing, which, of course, are much closer to the actual standard deviation.

The central issue of all these papers, even if you are not always using the same terminology, is compression versus shifting mortality and, here, I want to show you two clear examples, published by Siu Lan K. Cheung [myself and Graziella Caselli] in 2008. When you look at Japan, you can see that the modal age at death is steadily increasing over time from 1950 to the most recent years. During the first 30 years, the standard deviation or the mean deviation above the mode clearly decreased, illustrating a strong compression of mortality. But the last 20 years, the standard deviation or the mean deviation remained stagnant. Thus, Japan moved, it's very clear, from a strong compression of mortality to a shifting mortality scenario.

Now, if we look at Sweden, the compression of mortality is also clear, at least for the last 20 or 30

years. We observe a strong increase in the modal age at death and a strong decrease in the mean or the standard deviation above the mode. Therefore, for the last 20 years, a clear compression of mortality occurred in Sweden while Japan experienced during the same period a clear shifting mortality scenario. Here we have two clear pictures. If we look at France and Italy, it's not as clear. It seems that France and Italy are somewhere in middle. At the beginning there's a strong decrease in the standard deviation, but at the end it's still decreasing, but at a lesser rate. It seems we are in "something" which is intermediate. This is a little bit like what Emily Clay pointed out in her paper. To look at that, I used all available information in the Human Mortality Database (HMD), all available life tables, more than 4,000, and looked at the modal age. I made a scatter plot with the modal age at death and the mean deviation above it. On the first axis I put the modal age of death (M), and on the second axis, the life expectancy at the model age of death (e(M)), which corresponds to the mean deviation above the mode. We observe a huge increase in the modal age of death and a clear decrease in the mean deviation which occurred over time. But the relationship between the two indicators is not linear. It's clearly curvilinear. On the left of the graph when the modal age of death is moving from values circa 70 to 80, the mean

deviation strongly decreased. On the right, when we are in the area around 90, which is the case today in France, in Japan or in Switzerland, we can see that when the modal age of death is increasing, let's say, from 85 to 90, the decrease in the mean deviation is very weak. It's like we are going to a floor. Possibly when we consider the modal age of death increasing from 90 to 95, maybe the mean or the standard deviation will stop decreasing.

It seems, when we look at all the life tables available in the Human Mortality Database, that we moved from a situation of strong compression of mortality, in the past, when we moved from low modal age of death around 70 to 80, to a current situation where the decrease in deviation is very weak. The modal age at death may go increasing to 100 or more, but without any more decrease in the mean or in the standard deviation.

Eventually, I want to say one word about the case of the U.S., because, Jack, in your paper you say on the one hand we have Japan and the U.S., and on the other hand we have other countries like France and Australia. I don't think that the U.S. and Japan are in the same situation because as you can see on this slide, Japan is really leading the longevity revolution. Japan has a higher life expectancy compared to all other countries, and it is when life expectancy at birth was already quite high that we

observed the stagnation in the mean deviation above the modal age at death. The U.S. like Denmark or the Netherlands now is lagging beyond the other countries in term of longevity. It seems that in the United States, there is still an excess of premature mortality, responsible for the stagnation in the mean or the standard deviation around the mode.

I borrowed the next slide to Nadine Ouellette. This brings us to the issue of data quality. When we are looking at the limits of the human longevity or the mortality among the oldest old, it is complicated to use American data. As demographers, we have some difficulties trusting the data. This slide was presented at the IUSSP [International Union for the Scientific Study of Population] General Conference in 2009. It is almost impossible to explain the trend in the modal age at death in the United States without calling for data quality. Thus, I would be very careful before drawing conclusion on something like moving from the compression to the shifting mortality scenario.

Now, I want to come back in the final part of my talk on this idea of different survival curves used by James Fries in 1980 with the morbidity curve. This proposal is not elaborated at all in his paper, just one sentence under a figure. Ken Manton has been really instrumental, building in this first proposal to come a few years later with a

proposal published in 1984 by the World Health Organization, opposing three survival curves: the mortality curve and two hypothetical morbidity and disability survival curves. This framework offered the possibility to compute the life expectancy without disability and the life expectancy without morbidity as proposed a few years before by [Barkev] Sanders (1964) and [Daniel] Sullivan (1971). This framework is currently used in Europe to compute and compare total life expectancy with life expectancy without chronic morbidity and life expectancy without activity limitation for the 28 EU member states.

This is the right way to put in relation the rectangularization of the total survival curves, to the rectangularization of the survival curve without disability or without chronic morbidity, and to measure compression of disability or compression of morbidity. For the time being we can only do that in one country, in the Netherlands, which has kept the same design and the same questions for its social and health surveys for a long period of time, providing series for at least 25 years. In the Netherlands, life expectancy increased very slowly from 1985 to today, at least at the beginning. At the same time, the Netherlands experienced a strong increase in disability-free life expectancy, leading to a compression of disability (i.e., compression of the years lived with

disability). But during the same period, the Netherlands experienced a stagnation in life expectancy in good perceived health. However, as the concept of self-perceived health is subjective by definition, it is difficult to expect any trend. The big surprise in examining the graph is the strong decline in life expectancy without chronic disability. We don't think that the Dutch people are in much worse health today than they were in 1985, but they know much better about their health. Their level of expectation is possibly much higher, so their reporting is much closer to true health and chronic diseases than in the beginning. This is exactly why in the '80s we decided not to monitor population health using chronic diseases or self-perceived health, but using disability. Thank you for your attention.

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